The connexion between vital and physical motion : a conversation.

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THE CONNEXION BETWEEN

VITAL AND PHYSICAL MOTION:

A CONVERSATION.

C. B. Radcliffe
1881

C

THE CONNEXION BETWEEN VITAL AND PHYSICAL MOTION:

A CONVERSATION.

SCENE:

A Private Physiological Laboratory.

PERSONS:

A.—The Owner of the Laboratory.
B.—A Student who is working in the Laboratory.

A PRELIMINARY NOTICE.

The following pages are really little more than what the printers call a *proof* of a chapter—which may or may not be published—in which I have striven to give an amended *résumé* of an argument which has occupied my thoughts for many years and brought me too often into premature relations with printers and publishers. I should be very glad if these relations had never existed: I am not sure that at present I shall renew them so as to include the publishers ; and, in fact, all that I propose to do now is, in imitation of the practice of Cardinal Du Perron, to print a few copies for private circulation, and to distribute them in the hope that in this way I may become possessed of certain criticisms which may enable me to cope more conclusively with an argument concerning the substantial truth of which my mind has never at any time been darkened by the very faintest shadow of doubt.

C. B. R.

25 *Cavendish Square: Feb.* 1881.

A. You have been using the new quadrant electrometer, I see.

B. Yes. I want to know more about the phenomena of animal electricity which are brought into clearer light by means of this instrument, for, after what you told me the other day, I am very much disposed to believe that the key to the story of vital motion, which I am so wishful to find, is only to be found by seeking in this direction. The story of vital motion as told now-a-days has, as it seems to me, very little point in it.

A. You may well say so. This story has, in fact, as little point in it as the stories which were told in the distant days when *occult qualities* of one kind or another were thought to furnish a sufficient explanation for everything—when, for example, the answer to the question "quare opium facit dormire" which Moliere puts in the mouth of Argan in the "Malade Imaginaire," would have been listened to without a smile if it had been delivered in sober earnest before the examiners of a real faculty of medicine:—

> Mihi a docto doctore,
> Demandatur causam et rationem *quare*
> Opium facit dormire
> Et ego respondeo,
> *Quia* est in eo
> *Virtus dormitiva*
> Cujus est natura
> Sensus assoupire.

In order to explain the special power of opium

Argan calls in the help of an *occult quality* to which he gives the name of *virtus dormitiva:* in order to explain the special power of muscle or nerve you call in the help of *a vital property of irritability*, which is, to say the least, as much an *occult* quality as the *virtus dormitiva* itself. And this is a very unsatisfactory conclusion to arrive at. "To tell us"—the words are those of Newton —"that every species of thing is endowed with an *occult specific quality* is to tell us *nothing*." "To say that a phenomenon is *vital* is," as Whewell declares, "very prejudicial to the progress of knowledge by stopping inquiry *by a mere word*."* To decide that one set of phenomena is vital and another physical, as Coleridge points out,† "can, at the best, only be regarded as a hasty deduction from the first superficial notions of the objects that surround us, sufficient perhaps for the purposes of ordinary discrimination, but far too indeterminate and diffluent to be taken unexamined by the philosophical enquirer. . . . . . By a comprisal of the *petitio principii* with the *argumentum in circulo*—in plain English, by an easy logic which begins by begging the question, and then, moving in a circle, comes round to the point where it begins—each of the two divisions has been made to define the other by a mere re-assertion of their mutual contrariety. The physiologist has luminously explained $y + x$ by informing us that it was a somewhat which is the antithesis of $y - x$, and if we ask what then is $y - x$ the answer is the antithesis of $y + x$:—a reciprocation which may remind us of the twin sisters in the fable of the Lamiæ, with one eye between them, which

* "Philosophy of the Inductive Sciences," 2nd Edition, Vol. ii, p. 299.

† "Hints towards the formation of a more Comprehensive Theory of Life," by S. T. Coleridge. Edited by S. B. Watson, Churchill, 1848, pp. 21, 22.

each borrowed from the other as either happened to want it, but with this additional disadvantage, that the eye in the present case is, after all, but an eye of glass."

B. You must not assume that I am disposed to put trust in a vital property of irritability. I see as plainly as you do that nothing is to be gained by so doing—that a vital property of irritability is a baseless dream—that it is in every way a blunder to continue to regard such a property as furnishing any sort of key to the problem of vital motion. Indeed, as I have already said, I am disposed to agree with you in thinking that there is a close connexion between vital and physical work. So go on, I pray, and tell me more about your own views in this matter.

A. What I hold to be right views regarding vital motion began to be entertained when Galvani made the discovery which is commemorated in the following inscription on a marble slab over the doorway of a house —to which I have made more than one pilgrimage—in the Strada Felice (già Via Ugo Bassi) at Bologna:—

LUIGI GALVANI
in questa casa
di sua temporaria dimora
al primi di Settembre
dell' anno MDCCLXXXVI
scoperse dalle morte rane
LA ELETTRICITA ANIMALE
Fonte di maraviglie
a tutti secoli.

Two facts, then noticed for the first time, the one in the kitchen of this house, the other in a belvidere on the roof—which served the purpose of an electrical observatory—led to this discovery in this way. The first fact was this—that a number of frogs' legs which were being prepared as a dish for dinner, at no great distance from the place where Galvani and his nephew, Camillo Galvani, were experimenting with an ordinary electrical machine, were seen to jump whenever a spark was drawn from the prime conductor. The second fact was this—that some of these legs were also seen to jump, apparently without any electrical reason for jumping, after having been carried up into the belvidere and suspended by bent pieces of iron wire (of which one was passed through each pair so as to transfix the portion of spinal cord belonging to it) upon the iron stay which still passes horizontally across each of the arched openings of the belvidere at the height of about four feet above the parapet. When Galvani noticed the first fact, the thought instantly occurred to him that the legs which thus jumped in obedience to discharges of franklinic electricity would also jump in obedience to discharges of atmospheric electricity, and that, by so jumping, they would serve as excellent electroscopes in some investigations on atmospheric electricity in which he was then engaged: and in the full expectation that they would do so, he at once proceeded to take a number of them up with him into the belvidere, and to hang them upon the iron stay by the iron hooks, as I have just mentioned. He hoped that these natural electroscopes would prove to be far more sensitive than any of the clumsy artificial electroscopes which he was then using. He fully expected that the legs, thus suspended, would be thrown into a state of contraction by any gleam or flash of lightning; he did not expect that they would jump when the sky was, as it was then, as calm and clear and free from any apparent electrical disturbance as it could well be. He did not expect that he would have to record what he did and what he saw in these words:—" Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat: en motus in rana spontanei, varii, haud infrequentes. Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur et toties ferme quoties hujusmodi pressio adhiberetur."* For some time Galvani was not a little perplexed by what he saw. He saw that there was no reason to refer these contractions to the action of franklinic electricity. He saw that there was almost as little reason for referring them to the action of atmospheric electricity. He could not see, indeed, how electricity could have anything to do with them, for these two kinds of electricity were the only kinds of electricity then known. This he saw and only this, until, towards the close of the evening, it occurred to him that the limbs might have an electricity of their own, and that the contractions might be brought about by discharges of this electricity. "Yes, yes," I can imagine him saying at the time, "there is an electricity in living animal tissues, over-looked hitherto, which has to play an all-important part in muscular movement. It may be so: it must be so." At first he had little to say in support of this conclusion: at last he was able to state his case in such a way as to oblige many to agree with him, among others one who afterwards became greatly celebrated as Alexander von Humboldt, who had even then won for himself the right of speaking authoritatively on the subject by having dealt with it experimentally in the most exact manner, and who, in speaking of Galvani, was quite as ready to use words of praise as the writers of the inscription on the marble slab, for he writes:—"le nom

* "De viribus electricitatis in motu musculari commentarius," 1791.

de Galvani ne périra point: les siecles futurs profitéront de sa decouverte, et, comme le dit Brandes, ils reconnâitront que la physiologie doit à Galvani et à Harvey ses deux bases principales."* All along, however, there was one ear which was deaf to anything that Galvani had to say, and this was that of his colleague Volta. "I do not agree with you," we may imagine Volta saying to Galvani: "I also have gone over the experiments which you have instituted in order to demonstrate the reality of animal electricity, and I draw a different conclusion from them. What you attribute to the working of animal electricity I attribute to the working of another kind of electricity hitherto unknown—to a kind of electricity which has its origin in the reactions of heterogeneous bodies—of muscle with nerve, of muscle or nerve in one state with muscle and nerve in another state, of animal tissue with metal, of one kind of metal with another kind of metal, and so on." And in support of his opinion he was able, before he had done, to point, as he thought triumphantly, to his brilliant discovery of the voltaic pile and battery. Indeed, for several years the brilliancy of this discovery had the effect of diverting the attention of almost all physiologists from the less conspicuous phenomena of animal electricity. And thus it was, that the attempt to explain the muscular contractions which were witnessed by Galvani in the belvidere of the house in the Strada Felice at Bologna in September 1786, led on, not only to the discovery of animal electricity and of the connexion between this electricity and vital motion, but also to the discovery of voltaic electricity.

* "Experiences sur le galvanisme, et, en général, sur l'irritation des fibres musculaires et nerveuses." A. Humboldt: Traduit par J. F. N. Jadelot. 8vo. Paris, 1799, p. 391.

B. Is it not going a little too far to speak of Galvani as the discoverer of animal electricity? I should be disposed to say that the first actual step in this discovery was not made until Nobili detected the "frog-current" by means of the galvanometer which he himself had invented, and that it is not right to speak of this discovery as at all complete until Matteucci and Du Bois-Reymond had advanced much further in the same direction. What do you say?

A. Nothing in disparagement of Galvani certainly, and little more, unless you are in the mood to have a talk about the bearings of the discovery of animal electricity upon the interpretation of vital motion.

B. I am, I assure you, very much in the mood to hear all you have to say on this subject, and to put in a word or two of my own now and then.

A. You are, I know, familiar with the "muscle-current" and the "nerve-current."

B. I know this—that a current is generated in living muscle and nerve which is not met with in dead muscle and nerve, and that this current disappears in great measure whenever the state of rest changes into that of action. I know that in muscle and nerve alike this current tells upon the galvanometer in a way which shows that any point on the sides of the fibres is positive in relation to any point on the cross-section of the fibres. This current, I suppose, is a primary phenomenon.

A. For some time I have been in the habit of regarding the "muscle-current" and "nerve-current" as secondary rather than as primary phenomena, and at the present time I am more than ever disposed to abide by this opinion. Indeed, ever since I began to use the new quadrant electrometer of Sir William Thomson in investigating the electrical condition of living muscle and nerve, I have found myself almost compelled to believe that the electrical condition of muscle and nerve is one of *charge* during the state of rest, and one of *discharge* when the state of rest changes into that of action.

B. Almost compelled!

A. Yes. When a new quadrant electrometer is connected with a portion of living nerve or muscle so as to show in turn the electrical condition of the sides and the cross-sections of the quiescent fibres it is found that the sides are unequivocally positive and the cross-sections as unequivocally negative. In other words, the electrical condition of the fibres which is thus brought to light is one of *double charge*—is one, in fact, which seems to show that the fibres are in a state either of *open voltaic circuit* or of *double charge* like that of charged leyden jars—is one with which the idea of *current* is scarcely compatible.

B. There is, so far as I know, no reason why more importance should be attached to the *current* electrical phenomena of nerve and muscle than to the *statical* beyond this—that the evidence supplied by the galvanometer was the first to which attention *happened* to be directed.

A. Again: when the new-quadrant-electrometer is used so as to exhibit the electrical conditions of nerve and muscle in the opposite states of rest and action, it is found that the charge which is present in the former state is absent in the latter. It is, indeed, highly probable that the state of action in both nerve and muscle is attended by the disappearance of charge—is marked, in fact, by *discharge*.

B. You may, if you choose to do so, look upon the disappearance of current which is brought to light by the galvanometer when muscle or nerve passes from the state of rest into that of action as equivalent to discharge ; nay, you may even imagine that this current is only a *discharge bridled down into the quieter pace of the current* by having to encounter the high degree of resistance which is opposed to its passage in the very long and very fine wire of the coil which is necessary to detect the *current*, and that there would be *discharge* in place of *current* if the coil were made of a short length of thick wire like that which is used in thermometric investigations.

A. Without doubt.

B. Matteucci was right then in saying that a discharge analogous to that of the torpedo is developed when muscle passes from the state of rest into that of action.

A. There are, as Matteucci pointed out, sundry good reasons, anatomical and physiological, for believing that muscular action is accompanied by a discharge analogous to that of the torpedo. The nerves of the muscles agree with the nerves of the electric organs in originating in the same track of the spinal cord and in terminating in the same loop-like plexuses. The electric organs agree with the muscles in being paralyzed by the division of their nerves, and also in this, that, after being paralyzed in this way, they may be made to act by faradizing the nerve below the line of section. The electric organs agree with the muscles in being thrown into a state of involuntary action by strychnia. The electric organs agree with the muscles also in this, that they cannot go on acting without intervals of rest. And, lastly, the nerves of the electric organs agree with the nerves of the muscles in responding in the same curiously alternating way to the action of the "inverse" and "direct" voltaic current, if only discharge be taken as the equivalent of contraction.

B. Another good reason to the same effect may also be found in the fact that the *secondary* contractions which are set up in a *rheoscopic limb** by bringing the end of its nerve into contact with any part of the torpedo while its electric organ is in action are also set up by bringing the end of the nerve into contact with a muscle *or nerve* while it is in action.

A. Yes. You may certainly find in this fact an additional reason for thinking that the state of action in muscle, and in nerve likewise, is accompanied by a discharge analogous to that of the torpedo.

B. How is it that the fibres of nerves and muscles are charged electrically as they are found to be charged, and how is it that the charge is attended by lengthening and the discharge by shortening of the fibres in muscle but not in nerve?

A. It is highly probable—I shall have more to say upon this point and the next presently—that the electrical state of the earth generally is, not one of zero, but one of negative charge. It is highly probable that this state of negative charge is produced by the *inductive* action, across the air—which is the best of all dielectrics—of the positive electricity with which the medium of vacuum-like tenuity beyond the atmosphere would seem to be charged. It is highly probable that electricity, positive and negative, together with heat and some other modes of force, is generated in living bodies by the oxygenation of the force-fuel which is supplied in the food and in some of the residual materials into

* A *rheoscopic limb* is the hind leg of a frog from which all the parts above the knee have been snipped away except the sciatic nerve. It is placed in a dry test-tube, with the nerve brought out over the lip, and when in use the tube is held so as to allow the free end of the dangling nerve to rest lightly upon the body whose electrical condition has to be examined.

which the disintegrating tissues are resolved. It is, I think, far from improbable that the coatings of muscle-fibres and nerve-fibres may play the part of leyden jars, for these coatings are composed of a material closely akin to yellow elastic tissue—which tissue is a very bad conductor of electricity. It is, I think, far from improbable that the exterior of these coatings may be charged positively by the electricity which is liberated by the oxygenation of the force-fuel within the system, and that this + charge, acting across the dielectric coating, may *induce* an equivalent – charge on the interior of the coating. It is quite conceivable that the + electricity, which is liberated within the fibres when, by the action of induction, the – charge is developed upon the interior of the coatings, may escape through the nuclei of the fibres, as by so many vents—for the nuclei represent the parts in which the coatings are not developed—and so be added to the – charge of the earth, or lost by uniting with the – electricity which is liberated outside the fibres at the moment when the exterior of the coatings receives their + charge. And if so then it is possible to see how it may be that the fibres of nerve and muscle are positive at their sides and negative at their cross-sections, and how it is that in muscle the charge is attended by lengthening, and the discharge by shortening of the fibres. If the exterior of the coatings of the fibres of nerve and muscle are positive externally, and if these coatings are dielectric, it may well be that the fibres are negative at their cross-section. So far as it is made up of the cut ends of the coatings of the fibres, which coatings are + on one side and – on the other, the cross-section may be in a neutral state in consequence of the two opposite electricities which there come together neutralizing each other: so far as it is made up of the contents of the fibres —and this part after all may be by far the larger part— the cross-section may be negative, for if it be, as it may very well be supposed to be, that the semi-fluid contents of the fibres are capable of conducting electricity readily, the part formed by the cross-section of the actual contents must be in the main — by being in direct communication with every part of the negatively charged interior of the coating. And if the coatings of the fibres of muscles are charged in this way, and the coatings are elastic, it is quite supposable, not only that the coatings may be compressed, and the fibres elongated, by the mutual attraction of the two opposite charges with which the two surfaces of the coatings are charged, but also that the discharge of the charge may be followed by shortening of the fibres, for when the charge is removed which gave rise to lengthening of the fibres, the natural elasticity, which depends upon the action of the attractive force which is inherent in the physical constitution of the molecules of the coatings, must come into play. So it may be: nay so, indeed, up to a certain point, it must be. Nor need there be any great difficulty in understanding how it is that the fibres of nerves do not lengthen under the action of the charge, and shorten under that of the discharge, as do the fibres of muscles, for this difference may be nothing more than the necessary consequence of the coatings of nerve-fibres being somewhat less elastic than the coatings of muscle-fibres.

B. How do you account for the discharge of the torpedo? Is it a discharge like that of a leyden jar? Or is it the instantaneous current of high tension, which must be developed by induction whenever there is any movement in the electricity of the nerves of the electrical organs, and which may be intensified by the cells of the electric organ and the fibres and cells of the nerves having acted as condensers.

A. The discharge of the torpedo is, I think, more intelligible upon the latter supposition than upon the former. There is no charge to speak of in the electric organ of the torpedo in the intervals between the discharges: and, because its insulating powers are comparatively feeble, it is difficult to see how any powerful charge could be stored up in this organ, even for a moment. On the other hand it is easy to see how, at the moment of discharge, an induced current may be developed and intensified until it is as powerful even as the discharge of the torpedo. Any disappearance or reappearance of the electricity belonging to the nerves of the electric organ *must* lead to the development of induced currents in and around these nerves. And, most certainly, there is nothing unreasonable in the supposition that these currents may be intensified by the inter-action of the myriads of fibres and cells of the nervous system, and of the myriads of cells of the electric organ, for it is certain that a current is greatly intensified by multiplying the elements of the apparatus which is concerned, either in condensing it or in generating it. And if it be so with the nerves of the electric organ of the torpedo, in must be so also with any nerve or muscle whenever there is any variation in its electricity. Any such variation *must* lead to the development of an induced current in and around the nerve or muscle. It cannot be otherwise. And thus, by assimilating the discharge of the torpedo to the induced current which is developed in and around muscle and nerve wherever there is any variation in the electricity of muscle and nerve, it is easy to see that a discharge analogous to that of the torpedo is a necessary accompaniment of muscular action, and of nervous action also.

B. In comparison with the discharge of the torpedo the analogous discharge in muscle and nerve must surely be of very trifling importance.

A. I am not so sure of that. The discharge of the muscle or nerve *is* strong enough to cause *secondary* contraction in the muscles of a rheoscopic limb when the end of the nerve of this limb is brought into contact with a muscle or nerve in which a state of primary action has been set up: and, for the same reason, it *may be* strong enough to set up a state of action in muscle or nerve *within* the body. Indeed, I cannot help but think that the case of the rheoscopic limb exemplifies every case within the body in which nerve acts upon nerve or muscle, or muscle reacts upon nerve,—that in order to bring about this state of action or reaction within the body all that is necessary is, not organic connection between nerve and muscle, or between nerve and nerve, but simple approximation—that the agent which is directly concerned in bringing about this action or reaction is none other than the *induced current* which is developed in and *around* the nerve and muscle when either passes out of the state of rest into that of action, or returns from the state of action into that of rest.

B. By being *short-circuited* within the body I can also see that the induced current which is developed in and around nerve or muscle when the state of rest changes into that of action, or *vice versâ*, may appear to be much less powerful than it actually is—that it may be powerful enough to discharge the charge, which is present in nerve or muscle during the state of rest, by the mechanical shock which it gives to the nerve or muscle—that it may, in fact, be not much less powerful than the discharge of the torpedo itself; and I can easily imagine that the multiplication of the fibres in muscles, and of the fibres and cells in nerves and nerve-centres, may have the effect of intensifying the discharge of the charge belonging to muscle and nerve—that, in fact, the fibres and cells may be multiplied because the muscle, or nerve, or nerve-centre, has to play the part of a condenser.

A. Yes. There is, I think, every reason to believe that the discharge of the charge of a muscle or nerve may be powerful enough to cause a mechanical shock to a neighbouring muscle or nerve, which shock is sufficient to bring about a state of action in that muscle or nerve by discharging the charge which inhibits action in nerve and muscle, and which in muscle causes elongation of the fibres, so long as the muscle or nerve is at rest.

B. You have not come to a full stop, I hope?

A. No. I want to say something now about *the action of artificial electricity in vital motion*, and, while I have been silent, I have been sketching four figures which may save me a good deal of verbal repetition, and at the same time help to keep me to the point. You see by the figures what I am now thinking about.

B. I see that three of the figures, numbered 1, 2, and 3 relate to the subject of electrotonus, and that the fourth numbered 4, has to do with the action of the "inverse" and "direct" voltaic currents. Are you not somewhat bold in beginning with these difficult questions?

A. I may be so, but I think I see my way as plainly here as elsewhere—a little more plainly perhaps: at any rate I venture to make a beginning here.

B. Very well.

A. The two figures which I have numbered 1 and 2 have to do with the subject of electrotonus. I have, as

Figure 1.

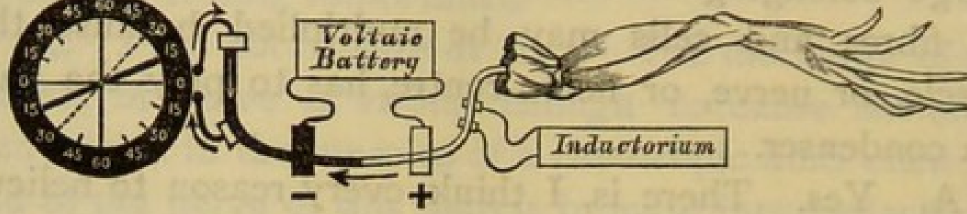
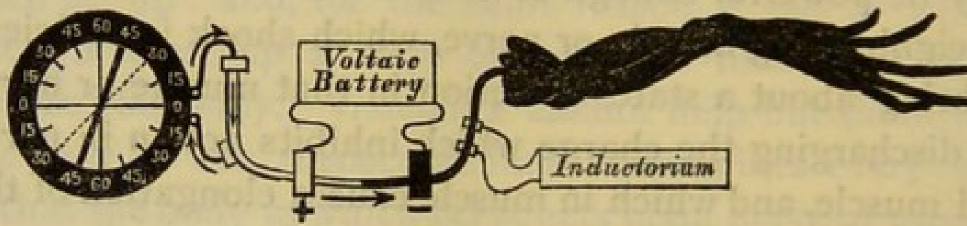


Figure 2.



you see, placed a rheoscopic limb, a galvanometer, a voltaic apparatus, and an inductorium as they are placed in the two ordinary experiments in which the phenomena of electrotonus are exhibited. The keys which are necessary for closing or opening or changing the different circuits are not represented, but all these you must imagine to be in their proper places. The two figures differ chiefly in the position of the voltaic poles, in the divergence of the needle of the galvanometer, and in the distribution of the white and black parts of the rheoscopic limb.

B. Why do you make certain parts of this limb black and leave others white, and why is the position of these parts transposed in the two figures?

A. I blacken certain parts in order to mark out the region of *cathelectrotonus*, and also to show that this region is charged negatively from the adjoining negative voltaic pole. I leave the other parts white in order to mark out the region of *anelectrotonus*, and also to show that this region is charged positively from the adjoining positive voltaic pole.

B. Is it so?

A. Yes. The two electrotonic regions are thus localized and thus charged. There can be no doubt as to the localization of the regions: there need not be any doubt respecting the charge, for the differences in question are easily demonstrable by a simple experiment with the new-quadrant-electrometer, or any other sufficiently sensitive instrument of the sort. Nor need there be any difficulty in arriving at the *rationale* of the phenomena, for these differences in charge are the necessary consequence of a bad conductor—the nerve is a very bad conductor—being placed between the voltaic poles as it is placed in the experiments under consideration.

B. I see so far clearly enough.

A. You may see clearly much further if only you will look a little steadily, and also understand how it is that the needle of the galvanometer should move as it is shown to have moved in the two figures, and how it is that the muscles should behave as they are found to behave during electrotonus and immediately after electrotonus.

B. You had better take nothing for granted except my mental blindness, so pray set to work and tell me all that you think necessary about the facts which are made known by the movement of the needle of the galvanometer on one side, and by the action or inaction of the muscles on the other side, together with your own conclusions respecting them.

A. The divergence of the needle of the galvanometer, which is caused by the action of the nerve-current, is found to increase under the influence of anelectrotonus, and to decrease under that of cathelectrotonus.

If, as it is supposed to have done in figures 1 and 2, the needle has diverged under the action of the nerve-current from the zero line, 0, to the dotted line pointing to 30°, before the state of electrotonus is set up, this divergence alters when the state of electrotonus is set up, to 15° if, as in figure 1, this state be that of cathelectrotonus on the side of the galvanometer, to 45° if, as in figure 2, this state be that of anelectrotonus. The nerve-current, that is to say, is found to be weakened under the action of cathelectrotonus and strengthened under the action of anelectrotonus. And why?

B. Because, I suppose, the voltaic current and the nerve-current *within the nerve* (the nerve-current *within the nerve* is from the cross-section to the side of the fibres) are in opposite directions in cathelectrotonus, and in the same direction in anelectrotonus. This is the explanation which is commonly held to be sufficient. Do you doubt it?

A. I am disposed to think that the voltaic action by which the nerve-current is modified is *unipolar*—that it is none other than that of the positive charge proceeding from the positive voltaic pole in anelectrotonus, that it is none other than that of the negative charge proceeding from the negative voltaic pole in cathelectrotonus. I can understand that the natural charge of the fibre of nerve and muscle will be diminished when this artificial charge is negative, and increased when it is positive, and that in this way, the nerve-current, which is proportionate to the electrical dissimilarities of the sides and cross-sections of the nerve-fibres, may be diminished or intensified. If the coatings of the fibres of nerve and muscle are really leyden-jars which, when charged naturally, are + externally and − internally, I can understand how it is that the charge may be strengthened in anelectrotonus by a + charge being imparted to the exterior of the coatings, and weakened or actually reversed in cathelectrotonus by the impartation of a – charge to this exterior. When a + charge is imparted to the exterior the natural + charge of the exterior will be strengthened directly by conduction, and at the same time the natural – charge of the interior will in an equal degree be strengthened indirectly by induction: and so it may be that the strengthening of the nerve-current, which is characteristic of anelectrotonus, is brought about. When a very feeble – charge is imparted to the exterior of the coatings the natural + charge of the exterior will be weakened directly by conduction, and at the same time the natural – charge of the interior will be weakened to an equal degree by the lessening of the inductive action which operates across the coating: and so it may be that the weakening of the nerve-current and muscle-current, which is met with in cathelectrotonus, will be brought about. And when a stronger – charge is imparted to the exterior of the coatings it is also easy to see how it may be, as it actually is, that the current in the galvanometer may be reversed, the needle going as far from zero as it went in anelectrotonus, only in the opposite direction: for in order to this all that is necessary is to suppose that the natural + charge of the exterior has been neutralized and supplanted by the artificial – charge imparted to the exterior, and that at the same time the change in the inducing charge has brought about a corresponding change in the charge induced on the interior. That is all. I shall have more to say in support of this view presently: now I merely ask you to remember what I have said when I come to reason about the phenomena with which I have next to do—the electrotonic phenomena, that is to say, which find expression in muscular movement or the contrary.

B. You have, I know, something new to tell me about these phenomena.

A. I have, but not until you have again listened to the old story.

B. I am quite ready to do so.

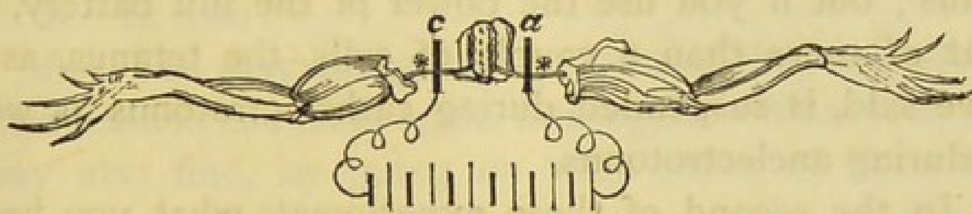
A. The materials for constructing the old story are found when the two experiments which are under consideration are repeated with what may be called a minimum amount of voltaic and faradaic power—when, that is to say, the voltaic apparatus is reduced to a single small element, or at most to two such elements, and when the inductorium is allowed to act upon the nerve so as only to produce a *single* muscular contraction of the very feeblest sort. The inductorium is put in action for a moment, (1) before the state of electrotonus is set up by closing the voltaic circuit, (2) during the continuance of this state, and (3) immediately after the electrotonus is at an end. And this is what happens. If the voltaic poles be arranged as in figure 1—the arrangement for producing anelectrotonus on the side of the muscles—there is, (1) contraction before the establishment of the state of anelectrotonus, (2) no contraction during the continuance of this state, and (3) contraction again, perhaps in a slightly intensified form, at the moment when the anelectrotonus is at an end. If, on the other hand, the voltaic poles are arranged as in figure 2—the arrangement for producing the state of cathelectrotonus on the side of the muscles—there is (1) contraction before the state of cathelectrotonus is set up, (2) more marked contraction, often considerably more marked contraction, during the continuance of this state, and (3) no contraction when the cathelectrotonus is at an end. And hence there is some reason to believe, not only that the activity of the nerve and muscle is suspended by anelectrotonus, and exaggerated by cathelectrotonus, but also that it is restored after anelectrotonus and suspended after cathelectrotonus.

B. Is the soundness of this belief to be called in question?

A. You will be able to judge for yourself if you master the particulars of two experiments which I have repeated many times, and always with the same results.

In the first of these experiments you place a galvanoscopic frog* as it is placed here—

Figure 3.



—that is, with the two legs stretching out in opposite directions, and with the middlemost points of the two nerves resting upon the poles of a voltaic battery consisting of half a dozen small cells. Then, while the circuit of the battery is still open, you place a drop of strong solution of common salt upon each of the nerves at the point indicated by the asterisk, and wait until a state of feeble tetanus is set up by the salt in both limbs. Then, while the tetanus is continuing, you establish the state of electrotonus by closing the voltaic circuit—of anelectrotonus in the limb on one side and of cathelectrotonus in the limb on the other side—and observe the result. And what you have to observe is this—that the tetanus which was going on in both limbs before the establishment of the state of electrotonus is

* A *galvanoscopic frog* is really no more than the two hind legs of the animal, without their skin, separated at the symphysis pubis, and with all the lumbar parts snipped away except the nerves and the bit of spine connected with them.

at once suspended as soon as this state is established. The contraction is not suspended by anelectrotonus and exaggerated by cathelectrotonus; it is suspended by cathelectrotonus no less than by anelectrotonus. This is the constant result when a battery of half a dozen cells is used in producing the state of electrotonus. If instead of using the full power of the battery you employ only a single cell, or at most two cells, you find that the tetanus may cease during anelectrotonus and continue, perhaps in an exaggerated form, during cathelectrotonus; but if you use the power of the full battery, or that of more than a couple of cells, the tetanus, as I have said, is suspended during cathelectrotonus as well as during anelectrotonus.

In the second of these experiments what you have to do is (1) to arrange two rheoscopic limbs as they are arranged in figures 1 and 2, (2) to allow the inductorium to act upon the nerve continuously so as to *tetanize* the muscles slightly, (3) to employ a voltaic battery consisting of half a dozen small cells, and (4) to make use of a conductor and a non-conductor in a certain way at a certain stage in the experiment. Before setting up the state of electrotonus you tetanize the muscles by faradizing the nerves; and then, while the faradization is still going on, you set up the state of electrotonus by closing the voltaic circuit, with the poles arranged at one time as in figure 1, at another time as in figure 2, and find, not that the tetanus is suspended during anelectrotonus and exaggerated during cathelectrotonus, as it was when you used only a single cell, or at most two cells in producing the electrotonus, but that it goes on with a certain degree of diminution, steadily and equally, in the two electrotonic states. Afterwards, while this mitigated tetanus is still continuing in both cases, you touch the tetanized muscles, or the nerve near them, first, with a glass rod or stick of sealing-wax, and then with an earth-wire, and find—what? That the tetanus is greatly exaggerated in the latter case, and not at all affected in the former case. In point of fact, the very marked muscular contraction which is brought about by the conductor and not by the non-conductor happens equally in the limb which is under the influence of anelectrotonus and in the limb which is under the influence of cathelectrotonus.

B. Is it so?

A. You may easily satisfy yourself that it is so. I have verified the facts again and again. Moreover, you may also find, by going on with either of these two experiments for a time, and by reversing the position of the voltaic poles so as to change the state of anelectrotonus for that of cathelectrotonus, or *vice versâ*, that the activity of nerve and muscle continues for a longer time under the action of anelectrotonus than under that of cathelectrotonus, and also that this power, after it has ceased under the action of cathelectrotonus, may be again and again restored, as it is restored in the so-called "voltaic alternatives," by reversing the position of the voltaic poles so as to change the state of cathelectrotonus for that of anelectrotonus.

B. Have you any satisfactory explanation to offer?

A. There is, so far as I can see, nothing very perplexing in any of the facts about which I have been speaking, and certainly the perplexity is not increased by the multiplication of the facts. Indeed, all that I have to do in order to arrive at what seems to be a satisfactory solution of the present problem, is to pursue the line of enquiry upon which I entered when speaking about the action of electrotonus upon the galvanometer, and to try and find the key to the electrotonic modifications of muscular movements in the reactions which must of necessity take place between the *natural charge* of the fibre of nerve and muscle and the *artificial charge*, + or —, which passes from the adjoining voltaic pole to the exterior of the fibre.

B. Have you any good reason to believe that these artificial charges can do the work you require them to do?

A. If you ask yourself how it is that a state of tetanus which is kept in check by electrotonus, as in the experiment last mentioned, is intensified by the touch of a conductor and not by the touch of a non-conductor, you may easily find in the answer a very sufficient reason for thinking that a *charge* is at work in electrotonus which is powerful enough to do real work. You may conclude that something is conducted away by the conductor which is not conducted away by the non-conductor. You can only think of a charge of electricity as the something which is at all likely to be thus conducted away or not conducted away. You have also unequivocal proof of the presence of a charge in the evidence supplied by the new-quadrant-electrometer. And therefore you have, as it seems to me, very sufficient reason for concluding that there is a charge at work in electrotonus which may be powerful enough to modify muscular action in the way in which it is modified in electrotonus.

B. I have nothing to say in contradiction. On the contrary, I am quite ready to follow you step by step in any course you propose to take.

A. In that case we shall, I think, have no difficulty in arriving at the end of our course without parting company. You have now to consider how a state of tetanus may be suspended by cathelectrotonus no less than by anelectrotonus,—how a state of tetanus which is thus suspended or kept greatly in check is at once developed in a very marked manner by touching the muscles or the nerves near them with a conductor, and not by touching them with a non-conductor,—how a single feeble contraction may be arrested by a feeble state of anelectrotonus, and exaggerated by an equally feeble state of cathelectrotonus,—how the nerves and muscles lose their activity much sooner under cathelectrotonus than under anelectrotonus,—and how this activity is again and again brought back by anelectrotonus after it has been banished by cathelectrotonus : and this task, after what has been said, cannot be very difficult.

It is not difficult to account for the action of anelectrotonus in suspending muscular contraction. The natural state of the coatings of the fibres, according to the premises, is one in which the exterior is + by conduction and the interior — by induction. The addition of + artificial charge to the exterior which happens in anelectrotonus must have the effect of intensifying the natural charge of the fibres, the addition of the + charge to the exterior necessitating by induction an equivalent addition of — charge to the interior. The case is one in which the fibres are more highly charged in the way in which they are charged naturally, and for that reason—if the premises are sound—less ready to pass into a state of action or contraction. The case, indeed, is one in which it is easy to see how contraction may be suspended by anelectrotonus. Nor is the case wholly different with cathelectrotonus when the electronizing voltaic power is more than that of one or two small elements. Here, the charge imparted to the exterior of the coatings is negative. Here, it may be supposed that the comparatively weak natural + charge of the exterior is supplanted by the comparatively strong artificial – charge, and that in this way a *reversal* of the natural charge of the coatings will be brought about, for the comparatively strong – artificial charge on the exterior must of necessity induce an equally strong + charge on the interior. In this way, it is quite conceivable that the coatings may receive a more powerful charge than that which is natural to them—that the charge may be intensified to the same degree in cathelectrotonus and in anelectrotonus, and with the same result so far as the counteraction of contraction is concerned, if the amount of charge be the same. And surely there can be no difficulty in supposing that the amount and the result may be the same, for the only difference between the two cases is that the relative position of the two charges which is natural in one case is reversed in the other case.

Nor is it difficult to account for the intensification of tetanus which happens in the case of anelectrotonus as well as in the case of cathelectrotonus when a conductor is used in a particular way. The conductor, as I have just shown, discharges a charge which antagonizes the state of muscular contraction—a charge which is not discharged by the non-conductor. The discharge, according to the premises, brings about muscular contraction by allowing free play to the elasticity of the muscular fibres. The charge of the fibres, as has just been seen, may be intensified in both forms of electrotonus: and, as a consequence of this intensification, there may have been an extra-degree of lengthening of the fibres. Indeed, there is a certain amount of evidence which goes to show that both electrotonic states, when sufficiently pronounced in degree, are attended by a lengthening of the muscular fibres. So it may be. And if it be so then the exaggeration of tetanus which is caused by the touch of a conductor in both the cases in question, and not by the touch of a non-conductor, may be nothing more than the natural result of the fibres recovering themselves from the extra-degree of lengthening which was brought about in electrotonus. That is all.

B. In this way, by showing that the charge and discharge are really operative, you make what was before little more than a matter of conjecture a strong probability. Until now, indeed, you had no sufficient reason for saying that the charge did lengthen the muscular fibres and that the discharge did permit them to shorten passively: now you have such a reason. But pray go on without having regard to my interruption.

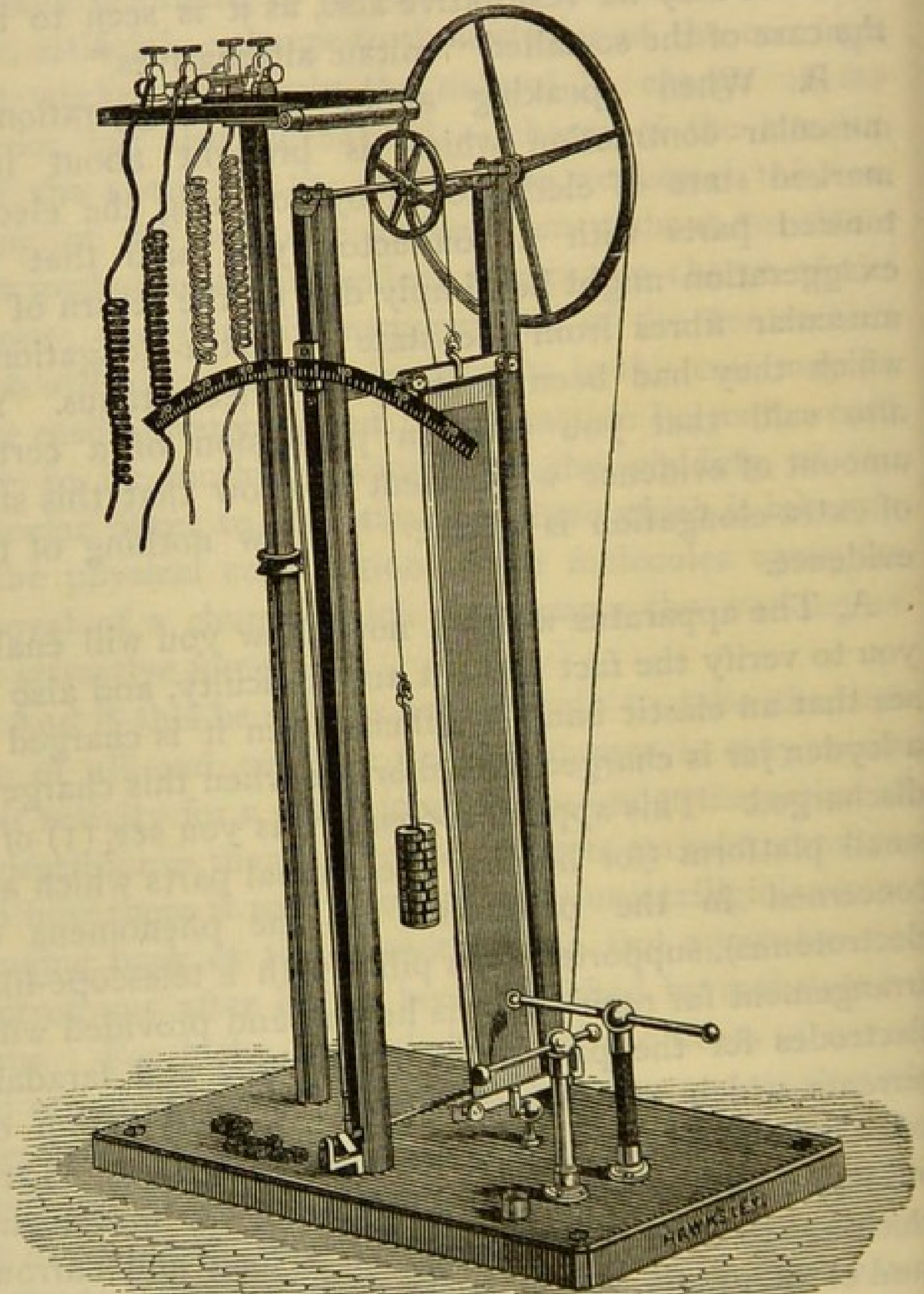
A. I have yet to speak about the suspension of contraction by anelectrotonus and the exaggeration of contraction by cathelectrotonus which happens when a single feeble contraction is experimented upon, and when a single small voltaic cell, or at most two such cells, are used in producing the state of electrotonus, and about the phenomena to which the name of "voltaic alternatives" has been given: and a few words will serve for this. It has been already seen how muscular contraction is suspended by anelectrotonus. Here a + artificial charge is imparted to the exterior of the coatings, and the whole charge of the coatings is increased, the additional + charge on the exterior inducing an equivalent — charge on the interior: and all that need be said is that the addition, small though it be, much smaller it may be than the natural charge, is sufficient to put an end to the contraction. Nor is it difficult to see how the contraction is exaggerated in cathelectrotonus in this case, if it be assumed, as it may be, that the artificial — charge imparted to the exterior of the coating is less in amount than the + charge naturally belonging to it. For here the addition of this weak, artificial — charge to the exterior of the coatings, must weaken, not only the natural + charge of the exterior, but also the natural — charge of the interior, for if the charge of the exterior be weakened this lessening of the inductive action from without to within must weaken to an equivalent degree the charge of the interior: and thus the exaggeration of the contraction which attends upon cathelectrotonus in this case may be more easily brought about, if contraction be, as it would seem to be, nothing more than the yielding of the muscular fibres to the attractive force which is inherent in the physical constitution of the molecules upon the removal of a charge which counteracts the working of this attractive force.

And if this be so it is not difficult to take the last step of all, and see how nerve and muscle may retain their activity for a much longer time under the action of anelectrotonus than under the action of cathelectrotonus, and how there is really nothing very unintelligible in the bringing back of this activity again and again by anelectrotonus after it has been banished by cathelectrotonus. For if the natural charge of the fibres of nerve and muscle is intensified in anelectrotonus and weakened in cathelectrotonus, in the way which I have just indicated, and if the activity of the fibres and the charge of the fibres are so inseparably connected as there is every reason to believe them to be, it is not altogether unintelligible that the action of anelectrotonus may be favourable to the continuance of this activity, and the action of cathelectrotonus unfavourable. And if so then it is scarcely necessary to take another step in order to see that the action of anelectrotonus upon the activity of the fibres of nerve and muscle which is preservative may be restorative also, as it is seen to be in the case of the so-called "voltaic alternatives."

B. When speaking about the exaggeration of muscular contraction which is brought about in a marked state of electrotonus by touching the electrotonized parts with a conductor, you said that this exaggeration might be simply due to the return of the muscular fibres from the state of extra-elongation in which they had been kept by the electrotonus. You also said that you were in possession of a certain amount of evidence which went to show that this state of extra-elongation is a fact. I know nothing of this evidence.

A. The apparatus which I now show you will enable you to verify the fact without any difficulty, and also to see that an elastic band lengthens when it is charged as a leyden jar is charged, and shortens when this charge is discharged. This apparatus consists, as you see, (1) of a small platform (for holding the animal parts which are concerned in the production of the phenomena of electrotonus), supported on a pillar with a telescope-like arrangement for regulating its height, and provided with electrodes for the passage of the voltaic and faradaic currents which are wanted,—(2) of a combination of axles and wheels and endless threads with a graduated arc and index for multiplying and indicating the movement which has to be measured,—(3) of a long and narrow band of thin india-rubber sheeting, provided (by rubbing it with plumbago or gilding it) with the conducting surfaces necessary to allow it to be charged as a leyden jar is charged,—(4) of certain clips and hooks-and-eyes and weights by which this band can be kept in its place and put gently upon the stretch,—and (5) of two move-

Figure 4.



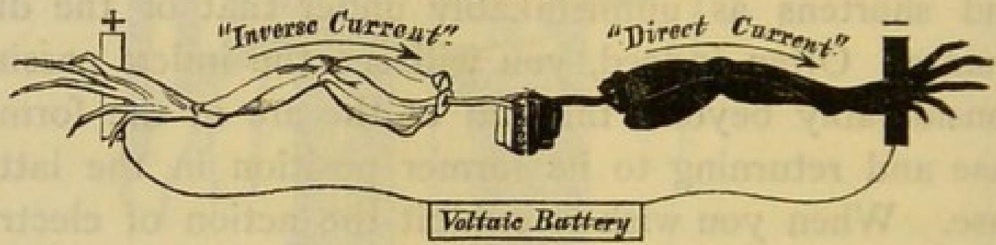
able rods, one insulated and the other not insulated, for facilitating the processes of charging and discharging.

When you want to see the action of the charge upon the band—the platform part of the apparatus is not in use in this case—you fix the band as you see it fixed and charge it from an ordinary electrical machine through the insulated rod, and then draw back this rod. When you want to discharge it you simply push the discharging rod forwards so as to make it touch the face of the band. If you do this the movement of the index before the graduated arc will show that the band elongates very unmistakably under the action of the charge and shortens as unmistakably under that of the discharge. Often, indeed, you will see the index passing considerably beyond the end of the arc in the former case and returning to its former position in the latter case. When you wish to exhibit the action of electrotonus upon muscular movement—you do not want the band in this case—you place a rheoscopic limb in the proper position upon the platform and fix it there, with its nerve properly arranged across the voltaic and faradaic poles, and with a thread fixed to the tendo-achillis and carried over the driving wheel of the apparatus to a weight which is just sufficient to put the gastrocnemius muscle gently upon the stretch. Then you use the voltaic battery and the inductorium in the ordinary way in which they are used in setting up and in testing the state of electrotonus, and when you have done this you have done all that is necessary to show that the gastrocnemius lengthens to a considerable degree—to 15° or 20° of the scale, it may be—and equally under the action of either form of electrotonus, and shortens to the same degree when this action is at end. You have only to repeat the experiments in order to be certain as to the facts.

B. How far do the facts which are made manifest in the experiment in which a galvanoscopic frog is exposed to the action of a voltaic current in the usual way agree or disagree with these conclusions?

A. The agreement is very close, and any disagreement is in reality more apparent than real. If you examine a galvanoscopic frog along which a voltaic current is passed in the usual way from foot to foot, as in figure 5, you find, if you examine the parts

Figure 5.



with a new-quadrant-electrometer, or any other sufficiently sensitive instrument of the sort, that the limb in which the current is ascending, or centripetal, or "inverse," is charged with positive electricity from the contiguous positive voltaic pole, and that the limb in which the current is descending, or centrifugal, or "direct," is charged with negative electricity from the contiguous negative pole. You find, indeed, a state of things which is represented in this figure, as it was represented in the two last figures, by leaving the parts which are charged positively white, and by making the parts which are charged negatively black.

If you allow the current to pass as in the figure you find that the limb in which the current is "inverse" and the charge positive preserves its activity for several minutes after the time when the limb in which the current is "direct" and the charge negative has lost its activity; and also that the parts which have lost their activity will again and again recover it if the poles be shifted so as to transpose the currents and the charges associated with them. You find, indeed, that the cases of the two limbs are precisely equivalent so far to the two cases of anelectrotonus and cathelectrotonus, the case of the limb in which the current is "inverse" and the charge positive being equivalent to that of the parts which are in a state of anelectrotonus, the case of the limb in which the current is "direct" and the charge negative being equivalent to that of the parts which are in a state of cathelectrotonus.

B. What have you to say about the behaviour of the muscles in this experiment?

A. The facts are not a little curious. There is no contraction while the current is passing steadily. At first, there is contraction at the closing of the circuit and at the opening as well; afterwards there is contraction at one or other of these moments only—at the moment of opening only in the limb in which the current is "inverse" and the charge positive, at the moment of closing only in the limb in which the current is "direct" and the charge negative. Later on, the contraction is found to come to an end in the limb in which the current is "direct" and the charge negative long before the time when it comes to an end in the limb in which the current is "inverse" and the charge positive. And still later you find that the contraction may be recalled again and again after it has taken its departure from the limb in which the current is "direct" and the charge negative, as it is recalled in the so-called "voltaic alternatives," by shifting the position of the poles, so as to make the current "inverse" and the charge positive where before the current was "direct" and the charge negative.

Nor is the explanation of these facts a matter of any great difficulty. At the closing and opening of the circuit there are movements of electricity which must lead to the development of "induced currents" in and around the fibres of nerve and muscle, and these induced currents may, I think, bring about the state of action in nerve and muscle by discharging the charge which antagonizes or inhibits the state of action—may, perhaps, bring about this state of action, by, as M. Chaaveau pointed out, the mechanical commotion which they set up in the nerve or muscle. At the closing of the circuit, also, a discharge which may lead to the same result by the same way, may be brought about by the meeting and neutralization of the opposite charges proceeding from the two poles. And, therefore, it is not altogether unintelligible that contraction should be at the moments of closing and opening the circuit, and not in the interval between these moments, for while the circuit is closed and the constant current is passing steadily the induced currents or discharges about which I have just been speaking are absent, and, besides this, the limbs are more or less occupied by the two voltaic charges which have been seen to have the power of antagonizing or inhibiting the state of action.

B. How is it that contraction is sometimes present and sometimes absent—present only at the closing of the circuit in the limb in which the current is "direct," present only at the opening of the circuit in the limb in which the current was "inverse"? After what you have said, I can see why contraction should be at the moment of closing the circuit, and also at the moment of opening it, but I cannot see why it should ever be limited to one of these moments in the way in which it is limited.

A. The fact in question is, I think, to be explained by remembering the different strength and direction of the two induced currents—by remembering that the induced current at the closing of the circuit is stronger than the induced current at the opening, and that the direction of the induced current agrees with that of the primary current at the closing of the circuit, and disagrees at the opening, and by assuming that at the time when these alternating contractions come to pass the *nerve* has become so far inactive as to be only capable of responding to the induced current which passes in the same direction as that in which motor impulses are transmitted along it to the muscles. For if it be so, then the contraction may be at the moment of closing the circuit in the case of the "direct current" simply because the direction of the induced current is then towards the muscles, and at the moment of *opening* the circuit in the case of the "inverse" current simply because the direction of the induced current is then towards the muscles.

B. How do you know that the action of the induced current is limited to the nerves in these cases?

A. Partly by what happens when the nerves are excluded from the circuit by bringing the muscles of the two thighs of the galvanoscopic frog together so as to make them touch closely (the muscles are much better conductors than the nerves, and, for this reason, they carry off the current from the nerves under these circumstances), or when the space between the two thighs is bridged over by a pair of compasses, or any other good conductor: and partly, by what happens when the action of the induced currents upon the nerve is traced in the direction of the sensorium as well as in that of the muscles. In the first case, the contraction which was happening at the closing of the circuit only in one limb, and at the opening of the circuit only in the other limb, is at once brought to the moment of closing the circuit in both limbs. The case is one in which it would seem that the muscles at this time respond, without any regard to the direction of the current, only to the *stronger* induced current which flashes through the circuit at the moment of closing the circuit. Nor is the inference which is deducible from the second case less unmistakable, for here—in the case, that is to say, in which the voltaic current is made to act upon a sciatic nerve which is in connexion with the sensorium as well as with the muscles—there is contraction at the closing of the circuit and not at the opening when there is pain at the opening of the circuit and not at the closing, and *vice versâ*. The case indeed is one in which, as you will see plainly enough if you will only look into the matter a little closely, there is contraction only when the induced current passes towards the muscles, and pain only when the current passes towards the sensorium.

B. You would not say, I suppose, that the activity of nerve or muscle is preserved and restored by the action of the "*inverse*" *current*, and that the action of the "*direct*" *current* is contrary to this?

A. No. I should say that the activity of nerve or muscle is preserved and restored by the action of the positive charge associated with the "inverse" current, and destroyed by the action of the negative charge which goes along with the "direct" current.

B. You do not leave much work for the *constant voltaic current* to do in these matters?

A. None at all. Indeed, I am disposed to agree with the late Mr. Gassiot in thinking that the constant voltaic current is revolvable into a movement in opposite directions of the two polar charges which are made manifest when the current is more or less obstructed in its passage by including in the circuit any bad conductor.

B. What about the history of *amœboid movements* and about *the movements of vibratile cilia?* Is there anything in the electrical history of these movements which is at all parallel to the electrical history about which you have been speaking?

A. All the protoplasmic bodies in which *amœboid movements* are exhibited—common amœbæ, the corpuscles in pus and mucus, the sarcode of fresh-water sponge, the æthalium of the tanyard, and the rest—are electrically in the same condition as water, or sculptors' clay, or inorganic matter generally.

B. Are you sure of this?

A. Yes; I have again and again assured myself as to the fact by making use of the new-quadrant-electrometer.

B. Do you mean to say that the electrical condition of the protoplasmic bodies in which amœboid movements are exhibited is one of *zero?*

A. No. The electrical condition of the atmosphere makes it highly probable that the medium of vacuum-like tenuity beyond the atmosphere is highly charged with positive electricity, and that this charge, by acting across the air, which is the best of all dielectrics, has *induced* an equivalent charge of negative electricity in the earth. The air is found to become more and more decidedly charged with positive electricity as the distance from the earth increases. This is the simple fact. The case is evidently one which agrees perfectly with the notion that the air is penetrated from without by a positive charge in precisely the same way as that in which the glass of a leyden jar is penetrated by such a charge: or rather, the case is one in which the atmosphere may be looked upon as at one and the same time penetrated from above and from below, as the two surfaces of the charged leyden jar are penetrated, by the two opposite charges. And, in fact, there is good reason why it must be so. For the case of the atmosphere is that of a very bad conductor placed between two good conductors, of which one is the earth, and the other the medium of vacuum-like tenuity beyond the atmosphere —for a vacuum *is* a very good conductor—a case, that is to say, out of which it is impossible to exclude the workings of induction, if only a charge be communicated to one of the conductors. In a word, I find it impossible to entertain the notion that there is a positive charge beyond the atmosphere from which the atmosphere receives its positive charge, without coming to the conclusion that this charge must, by acting across the dielectric air, *induce* an equivalent negative charge in the earth.

B. I see no reason why it may not be so. I also see that the earth may be thus charged and yet appear to be in a state of zero, for it could only be by certain parts of the earth ceasing to be good conductors that the charge of the earth could be differentiated so as to become apparent.

A. It is also possible to find in the air a sufficient reason for believing that the earth and the air and the ethereal medium beyond the air are all of them highly charged with electricity. In the lowermost layers of the atmosphere the potential is very much greater in amount than it would appear to be at first sight. The experiments of Sir William Thomson, in which either the portable electrometer or the water-dripping-collector with the divided-ring-electrometer were used in testing, show that at 9 feet above the ground the potential may be equal to that of 400 Daniell's-cells in fine weather, and to five or six times as much in foul weather, and also this—that in foul weather the potential at 1 foot above the ground may be as high as it is at 9 feet above the ground in fine weather. And these high indications of potential are but low in comparison with those which are met with a few feet above the lowermost layers of the atmosphere—with those, for example, which were met with in the case, described by Mr. Crosse, of a damp driving fog in November out of which, by means of about 500 yards of insulated wire raised on long poles above the topmost branches of the highest trees in his grounds at Broomfield, enough electricity was collected to charge beyond the point of discharge, twenty times in a minute, a leyden battery of fifty pairs, containing in all 73 feet of coating, and requiring, to charge it up to the point of discharge, 230 vigorous turns of the wheel of a 20-inch cylindrical machine—a battery the discharge of which was attended by a report as loud almost as that of a cannon. In point of fact there is good reason to believe that the potential which is made manifest in a violent storm of thunder and lightning, may be nothing more than that which naturally belongs to the higher region of the atmosphere in which the storm is raging.

B. A charge like this may serve to keep the molecules of the atmosphere in an aëriform state of repulsion even without the help of heat. Or rather it may be that electricity and heat are so far correlated as to enable either one to do the work of the other. And if any special reason be wanted for so thinking, that reason may, I think, be found in the fact that the potential and temperature of the air are inversely related to each other in the summer and winter months of the year, the potential being lower and the temperature higher in the summer months, the potential being higher and the temperature lower in the winter months.

A. It is also easy to go a step further in the same direction, and see that the charge in the extra-aërial ether from which the atmosphere receives its charge in the first instance, may set up a state of molecular repulsion in this medium which may be sufficient to keep it in the state of vacuum-like tenuity, and also, that this charge, by acting across the di-electric air, may induce in the earth a charge which may have much to do, along with heat, in keeping parts of the earth which would otherwise be solid in a state of more or less marked softness or fluidity. The induced charge must be equivalent in potency to the inducing charge, and the potency of the inducing charge is evidently very great—great enough in all probability to do all that the induced charge may have to do in causing softness, or liquefaction, or aërification.

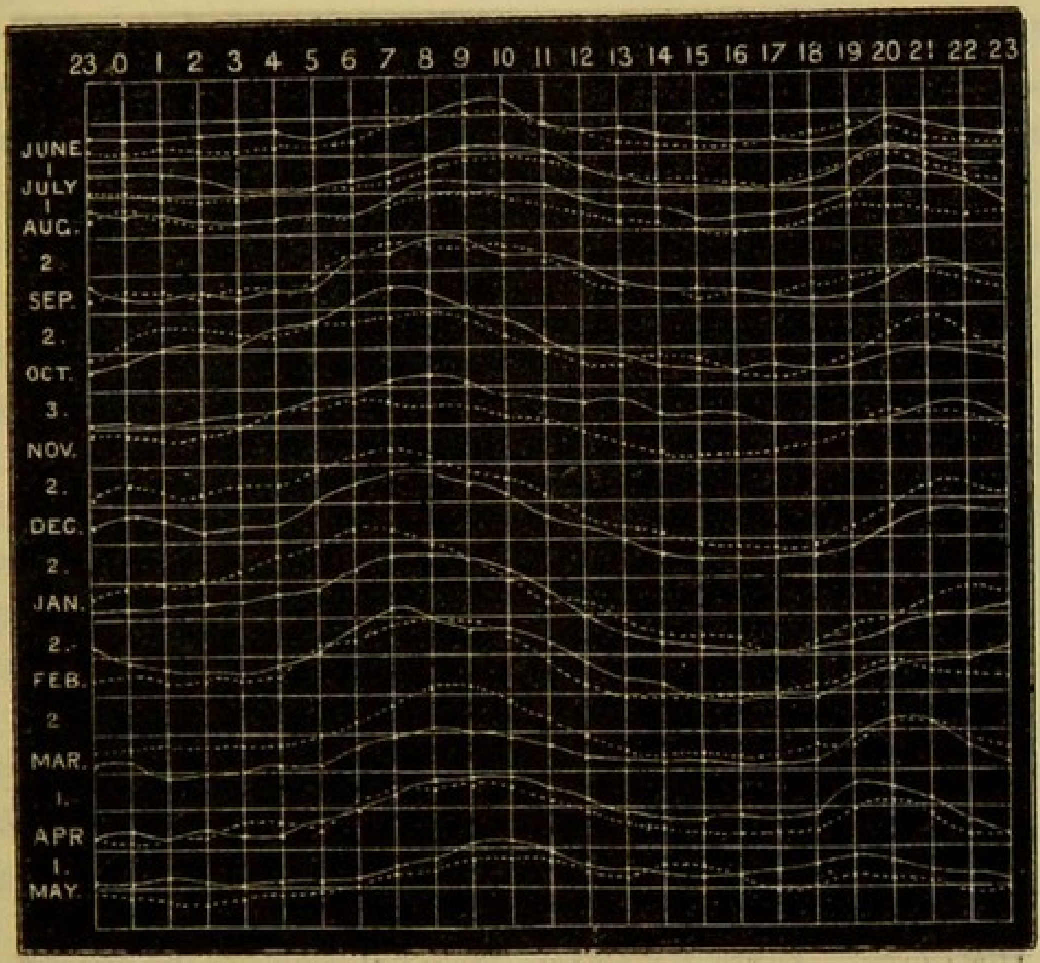
B. I have nothing to say in contradiction.

A. There is also reason to believe that there are every day two maxima and two minima of potential, one maximum between 8 A.M. and 11 A.M., the other between 7 P.M. and 11 P.M., one minimum between 3 P.M. and 7 P.M., the other between 11 P.M. and 3 A.M. The fullest statement of the actual facts is to be found in a paper* in which Dr. Everett, of Windsor in Nova Scotia, records the results of the observations in

* "Transactions of the Royal Society of London, 1868."

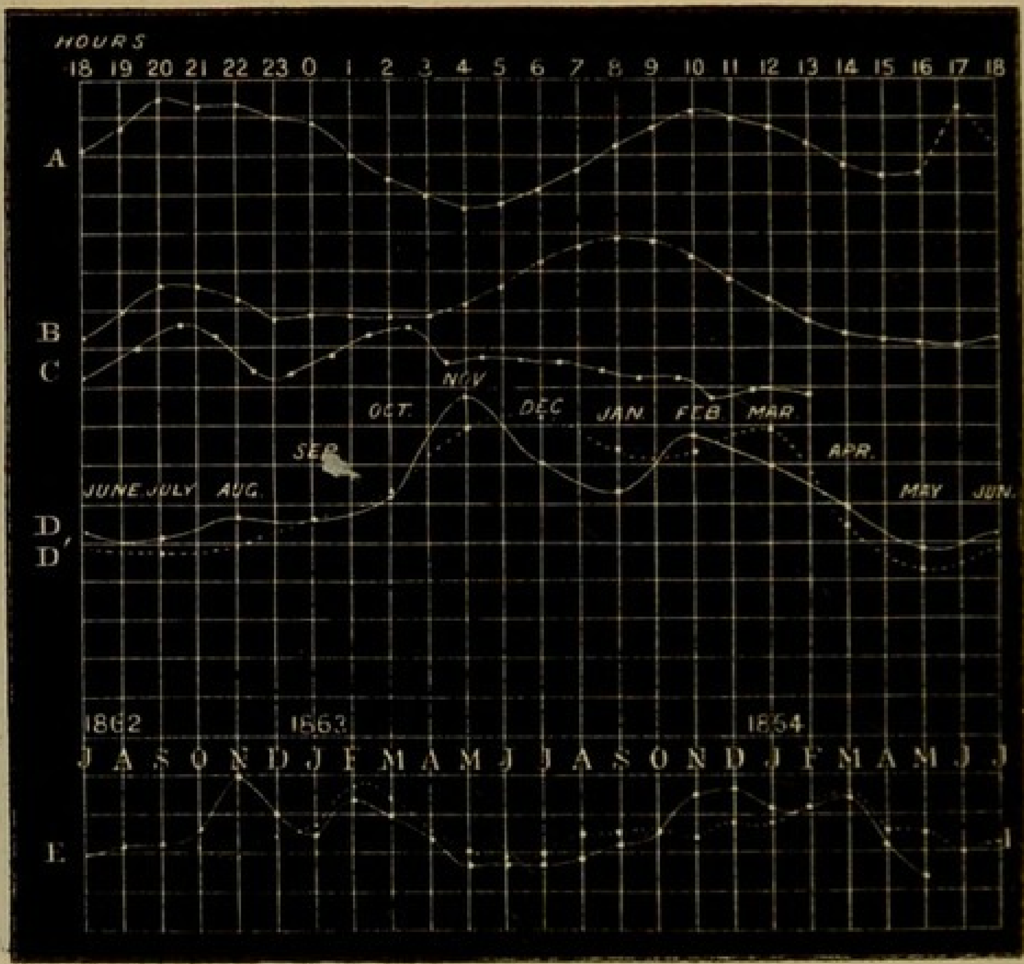
atmospheric electricity which were begun in the Royal Observatory at Kew under the direction of Professor Balfour Stewart, and carried on in America afterwards

Figure 6.



under his own superintendence—a paper of which the gist is given in the two diagrams of which I now show you copies. The first diagram, figure 6, gives the diurnal changes in atmospheric potential noticed at Kew hour by hour, and month by month, from June 1862 to May 1864 inclusive, the vertical lines figured at the top from 0, or midnight, to 23, or 11 P.M. (after the astronomical mode of reckoning) marking the time in hours, the horizontal curves to which the names of the months are prefixed doing the same for the months, the continuous curves for the months of the first year, the dotted curves for the months of the second. The second diagram, figure 7, in its different curves lettered from A

Figure 7.



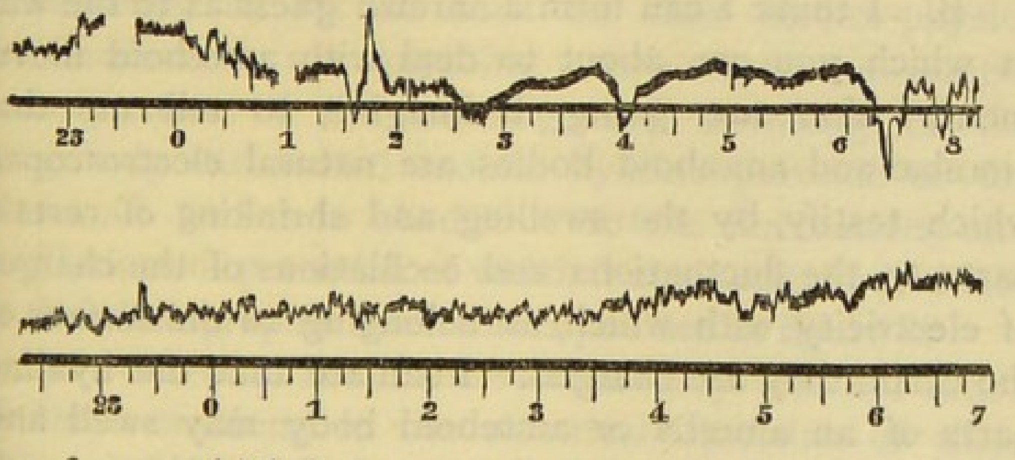
to F, gives a more comprehensive view of the facts set forth in the first diagram, and brings into prominence other important facts as well. A gives the mean of all the diurnal changes in potential noticed at Kew and recorded in figure 6: B is the mean diurnal barometric curve at Halle, as worked out by Dr. Everett from data in Kaemtz' "Meteorology:" C is the mean diurnal curve in potential relating to the observations made at Windsor in Nova Scotia: D, D′ and E are the mean annual curves in potential at Kew for the two years over which the observations extended, D belonging to the first year, D′ to the second, and E to the two years together ; while the remaining curve F gives the mean annual curve in potential at Windsor in Nova Scotia.

The electrical observations to which these curves relate were registered photographically by instruments with automatic action, and great care was taken in reducing them ; and, therefore, the fact of tidal movements in atmospheric electricity is put beyond question, as well as the close correspondence between these movements and the tidal movements recorded by the barometer.

These curves, however, do not represent all that has to be noticed in these matters. There are, indeed, waves in potential, as well as tides—waves which may prove to be of no secondary importance in our present enquiry.

The observations of Sir William Thomson leave no room for doubt as to the fact, and anyone who wishes to do so may realize in a moment what the fact actually is by looking at the figure, number 8, which I now show you

Figure 8.



--a figure which is an actual copy of a portion of the diurnal photographic record made at Kew of the working of a self-registering divided-ring-electrometer in connection with a water-dripping-collector. The sensitive paper (for the sake of economy, the same paper is made to do duty for *two* days) used in making these records is divided into hours, from 0, or midnight, to 23, or 11 P.M. The cylinder, around which the paper is wound, works by clockwork, and makes one revolution in twenty-four hours. The movements of the needle of the electrometer are photographed in the zigzag lines marked on the paper by a ray of light reflected from a mirror attached to the needle and moving with it. The point at which this needle would stand if it remained at zero is photographed in the straight datum lines by a ray reflected from a mirror attached to a suitable part of the immovable framework of the instrument. And thus, by comparing the zigzag lines, which represent the movements of the needle under changes in the potential of the atmosphere at the point where the water drips away from the nozzle of the water-dripping-collector, with the straight datum lines, it is at once made manifest that the potential of the atmosphere is continually agitated by an up and down wave-like movement.

B. I think I can form a shrewd guess as to the way in which you are about to deal with amœboid movement. You are going, I imagine, to tell me that amœbæ and amœboid bodies are natural electroscopes, which testify, by the swelling and shrinking of certain parts, to the fluctuations and oscillations of the charges of electricity with which, as belonging to the surface of the earth, they are charged. I can see that the hyaline parts of an amœba or amœboid body may swell and shrink under these oscillations and fluctuations much more perceptibly than the granular parts: and, as these hyaline and granular parts are distributed irregularly I can also see that the apparently irregular expansions and shrinkings of the amœba or amœboid body may be altogether regular. Is it not so?

A. You have certainly divined what I was about to say, and what, I think, ought to be said. In all cases amœboid movement would seem to be connected, not with the granular condition which is developed eventually in the protoplasmic matter, but with the hyaline condition which marks the very earliest stages of development. It is the least granular amœba, or pus-corpuscle, or mucus-corpuscle, or cornea-corpuscle, or bit of sarcode, which is most likely to be the seat of amœboid movement. Only let the granular condition be unequivocally developed in any of these bodies, and any such movement is at an end. It is with these bodies, indeed, as it is with portions of the æthalium of the tan-yard; for here it is certain that amœboid movement is witnessed, not when the jelly-like matter has become granular and comparatively solid, but only while it retains in great measure its original creamy consistence and remains hyaline under the microscope. In a word, there is no reason to believe that amœboid movements, so far at least as their electro-physics are concerned, are anything more than the simple consequences of the swelling and shrinking of the irregularly distributed mobile hyaline portions of the amœboid bodies in obedience to the fluctuations and oscillations of potential, of which there is certain evidence in the photographic lines which you see in figure 8.

B. And what about the *movements of vibatrile cilia?* Is there anything in the electrical condition of these cilia, or of the bodies supporting them, which will supply a key to this form of vital motion?

A. In order to account for the movement of vibratile cilia, all that *may be* necessary is to assume—that every fluctuation and oscillation of potential upon the surface of the earth is attended by the development of induced currents, and that the movements of the cilia are caused by these currents, as by a wind—that, in fact, the cilia themselves and the cells to which they are attached are as passive in the matter as are the blades of grass or the soil in which they grow when these blades wave backwards and forwards under the action of a breeze.

B. Is not all you have said about the *electrical history* of muscle and nerve in exact opposition to all we know about the *vital history* of muscle and nerve? You leave, as it seems to me, little work for *life* to do in vital motion. At all events, you cannot allow that life does its work in the way in which it is commonly supposed to do it.

A. I cannot allow that the way in which *life* is supposed to do its work in vital motion is the right way. I do not believe that muscle is endowed with a special vitality which, under the action of certain *stimuli*, finds expression in muscular contraction. I do not believe that the nerve-centres act upon the muscles by, as it were, supplying stimulating gushes of nerve-life, and that the power of repeating these gushes is directly proportionate to the supply of red blood to these centres. For what do I find when I look into the matter closely? I find that the contraction which may be produced in the hind leg of a frog by faradizing the sciatic nerve are more marked when the animal is decapitated, still more marked when the spinal cord is cut across in the middle of the back, and most marked of all when the muscles are altogether cut off from the spinal cord by dividing the sciatic nerve high up in the ham. I also find that the disposition to muscular contraction is inversely related to the supply of arterial blood to certain nerve-centres. In other words, I find reason to believe that the natural action of these nerve-centres upon the muscles is to prevent, or antagonize, or inhibit muscular contraction rather than to cause it—that "the morbid or exalted state of irritability" which is supposed to lead to exaggerated muscular action is associated with diminished vitality rather than with a contrary state of things. And so, as you will easily see, I am to a certain extent able to assimilate the action of nerve-power in vital motion to that of electricity.

B. Not so easily as you suppose. Indeed you had better assume that I am likely to see little to the purpose unless you take the trouble to tell me what facts I am to attend to and what inferences I am to draw from them.

A. In that case the first fact to be attended to particularly is that to which I have just alluded. You know the fact?

B. No.

A. You may soon acquire the knowledge, for all you have to do is—to fix a frog upon the frog-plate in the ordinary way, to attach one of the legs by the ankle to an apparatus by which the degree of contraction in the muscles of the leg may be measured, to faradize the sciatic nerve with currents which are only just strong enough to produce feeble contractions in these muscles, and then, being careful to use currents of the same strength throughout the experiment, to watch the apparatus which records the degree of contraction and note the result before decapitation, after decapitation, after dividing the spinal cord in the middle of the back, and after cutting across the sciatic nerve high up in the ham. If you do this you will find, speaking roughly, that the degree of contraction, which may be reckoned as 1 before decapitation, may be reckoned as 2 after decapitation, as 3 after dividing the spinal cord in the middle of the back, and as 4 after dividing the nerve outside the spinal cord. The disposition to contract in this case becomes greater and greater as the muscles are more and more cut off from the great nerve-centres. In other words, the disposition to muscular contraction is, as it would seem, inversely related to the supply of nerve-power from the great cerebro-spinal nerve-centres to the muscle.

B. Very well.

A. The second fact to which your attention may be directed is this—that violent general convulsion is brought about by profuse bleeding, by sudden strangling, and by closing the great cerebral arteries as is done, for example, in the experiments of Kussmaul and Tenner. In each of these cases the convulsion is evidently brought about in the same way, that is, by arresting the supply of red blood to certain cerebral nerve-centres. So long as the proper action of these centres is kept up by a sufficient supply of red blood there is no convulsion; as soon as this proper action is arrested by shutting-off this supply there is convulsion. This is the simple fact. And this being the simple fact, the only conclusion to which I can come is, that convulsion is prevented by the natural action of these great nerve-centres—that this action is exercised, not in causing muscular contraction, but in counteracting or inhibiting it.

B. I have nothing to say to the contrary. I am quite disposed to agree with you in thinking that the convulsion which is brought about by bleeding and strangling is due to one and the same cause, that is, to the lack of red blood. Indeed, I have never been able to bring myself to suppose that the convulsion connected with death by strangling is caused by the carbonic acid contained in the black blood (with which the arteries as well as the veins are then filled) having acted as a *stimulus* to any nerve-centre.

A. A similar conclusion to that which is drawn from the history of the convulsion connected with bleeding, with strangling, and with the experimental closure of the principal cerebral arteries, may also be drawn from the history of ordinary epilepsy and epileptiform convulsion. The immediate precursor of the epileptic paroxysm is a sign which it is somewhat difficult to catch—corpse-like pallor of the countenance. M. Delasiauve was the first to notice this phenomenon, and M. Trousseau properly insists upon it as a mark which distinguishes true epilepsy from feigned epilepsy. "Il est une signe," he says, "qui se produit au moment de la chûte, qui n'est imitable pour personne, c'est la pâleur très prononcée cadaverique qui couvre pour un instant la face epileptique. Nous ne la voyons pas, parceque nous arrivons trop tard, àlors que la face est dejá d'une rouge tres prononcée." In fact, the general form of the epileptic or epileptiform paroxysm begins in the same way as the partial form to which the name of *petit mal* is generally given, for cadaverous pallor of the countenance is found to hold a very conspicuous position among the initial symptoms in this latter case. In the actual epileptic or epileptiform paroxysm, the staring, squinting, out-starting eyes, the black and bloated countenance, the guttural sounds suggestive of strangling, and the spasm-bound chest, show plainly enough what is happening. The state which accompanies the convulsion is evidently that of suffocation. It is as if the unhappy sufferer were prevented from breathing by the strong gripe of some invisible fiend. Nor is it really otherwise in those varieties of the disorder, partial or general, in which the face remains pale and shrunken from the beginning to the end of the paroxysm, for here the face has always a ghastly pallor or lividity which shows very plainly that the convulsive symptoms are accompanied by some grave interruption in the proper aëration of the blood.

In some cases the pulse at the wrist and elsewhere is almost or altogether imperceptible from the beginning to the end of the fit; in others, the pulse rallies speedily, and when the convulsion is at its height, it is at once hard and full and frequent. How then is this? What is the true meaning of this state of seeming vascular over-action? The common belief on the subject is, that an increased quantity of *red* blood is pumped into the arteries during the paroxysm, and that this increased quantity of red blood produces the convulsion by provoking a state of increased functional activity in one or other of the great cerebral nerve-centres, and not long ago the late Professor Schroeder van der Kolk gave distinct expression to this belief. In reality, however, the pulse acquires power under these circumstances because the condition of the circulation at the time is one of suffocation, and for this reason simply. For what is the condition of the circulation in suffocation? It is *not* one in which, as is generally supposed, the arterial pulse fails rapidly for want of blood, and the venous system as rapidly becomes gorged with black blood: on the contrary, it is one in which the arteries go on filling at the expense of the veins, and in which the pulse in the arteries becomes harder and fuller, as the colour of the blood in the arteries is found to change from red to black, because the black blood does not find its way through the capillaries as easily as red blood. Evidence to this effect is abundantly supplied in the experiments of Reid and Draper and others. It is indeed certain that the strong and full pulse of the epileptic or epileptiform paroxysm is nothing more than the natural pulse belonging to the state of suffocation which obtains at the time—the pulse of black blood, the *apnœal pulse* as it may be called.

Over-activity of the circulation, indeed, forms no part of the history of epilepsy or epileptiform disease. Instead of predisposing to these disorders an over-active state of the circulation would seem to have a contrary effect, for it often happens that fits of daily recurrence are suspended by fever or inflammation. And certainly nothing to the contrary is to be gathered from the state of the circulation in the inter-paroxysmal period, for here, if anything out of the common is noticeable, it is sure to be some indication of defective vascular vigour—a pulse easily flagging, hands and feet readily becoming cold and clammy, and the like.

B. Is not a contrary conclusion to be drawn from the history of the convulsion which is associated with certain fevers, and with certain forms of inflammation of the brain in early life?

A. No. In the fevers of infancy and early childhood, in the exanthemata more especially, convulsion not unfrequently takes the place which is occupied by rigor or subsultus in the fevers of later years. The place of the convulsion, that is to say, is in the cold stage before the hot stage, or in the stage of sinking after the hot stage, but not in the hot stage itself. It is, indeed, as if there were something altogether uncongenial, or even incompatible, between the hot stage and the convulsion. Nor is convulsion a common symptom in inflammation of the brain or its membrane. Now and then, in children especially, convulsion may happen at the onset of the disorder, in the cold stage before the hot stage, or at the end of the disorder, in the stage of sinking after the hot stage, when the patient has all but ceased to strive in the "struggle called living;" but, so far as my experience goes, it never happens during the time when general febrile reaction with determination of blood to the brain is fully established. Nor do I find anything in the relations of convulsion to the state called "exalted or morbid irritability" which is at all calculated to invalidate these conclusions. For what is this state? It is not inflammation: it is not fever: it is some undefined and negative condition occurring frequently in teething, in worm disease, in "spinal irritation," and in many other cases—a condition in which the patient is greatly wanting in vital power, and for which *nervous exhaustion* would seem to be as good a name as any—a state, in fact, which is more readily accounted for on the supposition that certain nerve-centres are more blanched than they ought to be than upon the contrary supposition that they are more blood-shot than they ought to be.

B. Are you at liberty to assume that the *cold stage* of fever or inflammation is really marked by wanting activity in the circulation? Does not the high temperature in this stage point to a different conclusion? The sensation of cold is, as the late Dr. Parkes pointed out, merely a subjective symptom. In ague, where the cold stage is most marked, the skin during the *cold stage* is shrivelled and shrunken and goose-like, the nails are blue, and the countenance is pale, but the temperature in the axilla or in the mouth, even before the rigors commence, is considerably above the natural standard always, and may be upwards of 106° Fahr.—is, or may be, in fact, as high, or nearly as high, as it is afterwards in the *hot stage*. What do you make of this fact? Does it not supply a reason for thinking that there must be a state of over-activity of the circulation somewhere—for thinking that this state of over-activity may be in one or other of the great nerve-centres, and that the convulsion or rigor may be closely connected with it?

A. Presently I shall be better able to deal with this question; now, I will only say that the open pupils and the pallid and shrunken countenance, which mark the cold stage, supply what are, to my mind, very conclusive reasons for thinking that there is no over-active condition of the circulation during the cold stage in the great nerve-centres which have specially to do with the production of convulsion.

B. I must allow that there is nothing like "a rush of blood to the head" in the cold stage, and that it is difficult to connect the increase of heat which may then be manifested with over-activity of the circulation in the head or elsewhere. Moreover, I cannot overlook the fact that there is not unfrequently, as in typhus fever, a rapid rise of temperature in the moribund state.

A. The temperature may rise very considerably in the moribund state; and it may also go on rising for several hours after actual death. It is often so in tetanus, in apoplexy, in cerebral congestion, in cholera, in yellow fever, and in other cases also. In one of the cases of tetanus recorded by Dr. Wünderlich, for example, the notes of the temperature run thus:—

			Fahr.
During life on	July 24, 1861	...	102°
	„ 25, „	...	102°
	„ 26, „	9 A.M.	104·45°
	„ „ „	6 P.M.	103·55°
	„ „ „	9·20 P.M.	110·1°
Death at ...	„ „ „	9·35 „	112·55°
After death ...	2 minutes ...	...	112·77°
	5 „ ...	...	113°
	20 „ ...	...	113·22°
	35 „ ...	...	113·55°
	55 „ ...	...	113·67°
	1 hour ...	...	113·65°
	1 „ and 10 minutes		113·22°
	1 „ „ 30 „		113°
	1 „ „ 40 „		111·8°
	6 „ ...	...	106·25°
	9 „ ...	...	104°
	12 „ ...	...	102°
	13½ „ ...	...	101°

The temperature of the ward at the time of death was 77° Fahr.

The patient in this case was a butcher, aged 27. The disorder, which was idiopathic or rheumatic tetanus without any peculiarity as to symptoms, ran its course in five days, death happening in the state of exhaustion following a bout of spasm of no special severity, after an earlier change of short duration in which there was some delirium, with marked abatement of the spasms. Putrefaction was unusually rapid. The brain was healthy;

the spinal cord was here and there injected and considerably disorganized.

Here then is a case of tetanus—and it is in no way exceptional—in which no reason is to be found for connecting the rise of temperature either with over-activity of the circulation, or with excess of muscular action. The circulation is at a standstill after death, and almost at a standstill in the moribund state: the muscles are at rest after death, and there was a marked abatement of the spasms in the moribund state: and therefore, the case is one in which there is, as I have said, no reason for connecting the rise of temperature either with over-activity of the circulation, or with excess of muscular action. What do you say?

B. I have always thought that the increased temperature of tetanus was due either to over-activity of the circulation, or to excess in muscular action, to one or other cause or to both together, and if I must give up this notion I am at a loss what to say. What do you say?

A. I am disposed to think that this increased generation of heat has to do with a change in the central grey matter of the spinal cord or brain by which vaso-motor nerves or centres have been damaged. I am disposed to think that the capillaries may have become injected by a vaso-motor paralysis brought about in this way, and that molecular changes manifesting themselves in the increased development of heat may go on in the injected parts, as long as the blood remains fluid, even after the time when the circulation has been brought to an end by death. And there is, as it seems to me, good reason for so thinking in the fact that vaso-motor paralysis resulting in increased development of heat, without any over-activity in the circulation, is produced by mechanical injuries to certain parts of the spinal cord, and in the fact that in this very case of the butcher the cord was found to be damaged, by being here and there injected and considerably disorganized. And, therefore, it is not improbable that the increased development of heat which happened in this case in the moribund state and after death, and which, in fact, is no very unusual occurrence in cases of tetanus, may be consequent upon a damaged state of the cord. Am I not right in saying so?

B. I do not venture to have an opinion of my own yet.

A. I am also disposed to think that the inflammatory state of the cord of which there were traces after death in the case of the butcher and in many other cases of tetanus is *not* connected directly with the spasms of tetanus. Spasm is not a symptom of myelitis. Signs of myelitis are not always present after death from tetanus. In the case of the butcher there was abatement of the spasms before death. And, in fact, I am disposed to think that this inflammation of the cord may have had something to do in abating the spasms in this very case—that the spasms are connected with the stage of irritation of the cord which precedes the development of inflammation of the cord—that the signs of inflammation of the cord are usually absent in cases of tetanus because death usually happens before the development of true inflammation—that the state of the cord during the spasms of tetanus is more likely to be blanched than bloodshot, because the vessels are more or less contracted in the state of irritation which precedes inflammation—and that the spasms are prevented or antagonized or inhibited when the state of inflammation of the cord is fully developed.

B. How do you get over the fact that sharp tetanic spasm is a characteristic symptom of acute inflammation of the membranes of the spinal cord?

A. By denying the fact. Sharp tetanic spasm is *not* a characteristic symptom of acute spinal meningitis. What I find in this disorder are fits of sharp pain in the spine, and extending from the spine to the extremities, accompanied by fits of muscular stiffness in the painful parts, with intervals of comparative or complete freedom from pain and stiffness. The fits of pain and stiffness are evidently brought about by movement, and the stiffness is as evidently an instinctive act of muscular contraction to prevent pain by preventing the movements which produce pain. Between this occasional stiffness and the abiding spasms of tetanus there is little or nothing in common. Indeed, it is scarcely too much to say that the pain and stiffness of acute spinal meningitis may be kept altogether in abeyance if only the patient can contrive to keep perfectly still.

B. I have seen one well marked case of acute spinal meningitis in which it was without doubt as you say.

A. It is also more than difficult to look upon the local inflammation of gout as essential to the existence of the racking pain of this disorder. "About two o'clock in the morning," says Sydenham, who from personal experience knew full well what he ought to say, "the patient is awakened by a severe pain in the great toe, or more rarely, in the heel or ankle or instep. This pain is like that of a dislocation, and yet the parts feel as if cold water were being poured over them. Then follow chills and shiverings and a little fever. The pain which was at first moderate becomes more intense; and with its intensity the chills and shivers increase." After tossing about in agony for four or five hours, often till near day break, the patient suddenly finds relief and falls asleep. Before falling asleep, the only visible change in the tortured joint is some fulness in the veins; on waking in the morning, this part has become swollen, shining, red, tender in the extreme, and more or less painful, but this pain is as nothing in comparison with the torture of the night past. It seems, indeed, as if the pain which now exists must be referred to the mere tension and stretching of the inflamed ligaments, for it may be relieved, or even removed by judiciously applying support to the toe and to the sole of the foot. On the night following, and not unfrequently for the next three or four nights, the racking pain in all probability returns, re-appearing and disappearing suddenly, or almost suddenly, about the same time, and resulting in the discovery of additional inflammatory swelling upon awaking in the morning. The inflammation is attended by no fever, or by very little; or if it be otherwise, as happens occasionally, and the inflammatory fever runs higher than usual, *the characteristic pain is less urgent than usual.* Dr. Garrod points out this latter fact in his excellent work on gout, and says that he has met with several instances in illustration of it. From its history, then, it would seem as if the inflammation of gout were not essential to the characteristic pain of gout—as if the pain went hand in hand with the rigors which are preliminary to the development of the inflammation. Nay, it would even seem as if the pain were put an end to by the establishment of the inflammation—as if, in fact, the pain were antagonized by the inflammation rather than favoured by it. Moreover, the suddenness with which it begins and ends in the majority of cases must be looked upon as a reason for referring the pain to the category of neuralgia—a category in which, to say the least, it is not a little difficult to find any place for inflammation.

And so likewise with the pain of a neuralgic character which accompanies inflammation of any other sort in any other part. The pain is connected with the state of irritation which precedes the inflammation and not with the actual inflammation. It is tenderness, or else pain which is obviously connected with tension or pressure, not neuralgic pain, which has to do with the actual inflammation: and as in gout so in all these cases there is reason to believe that the latter pain is suspended by the inflammation. In a word, the history of neuralgic pain has as little to do with actual fever or inflammation as the history of convulsion or spasm or tremor.

B. I can see now, in some measure at least, how the part which "nervous influence" has to play in vital motion may be assimilated to that which is played by electricity. I can see that this vital power, if it be one, is depressed to a very great degree by the withdrawal of red blood from the "nerve-centres" whenever vital motion is manifested in the form of convulsion, or spasm, or tremor, or pain of the nature of neuralgia. I can see, indeed, that nervous influence may have an actual power of counteracting or inhibiting involuntary vital motion—that such motion is connected with the absence and not with the presence of such influence. In a word, I can see enough to make me ready to believe that the part which "nervous influence" has to play in the production of vital motion may be so far transferred to electricity as to leave little or nothing for this "influence" to do in the matter.

A. All the work which is supposed to be done by "nervous influence" in vital motion is, I think, done by electricity. I believe that the nervous system is charged by the electricity which is generated in the nerve-centres and in the nerves mainly by the action of the red blood—that this generation is proportionate to this supply—that this charge counteracts or inhibits vital motion as long as it is properly kept up—and that involuntary vital motion is the consequence of the disappearance of this charge in discharge. That is all. I cannot dispense with the action of the will: I can easily dispense with the action of "nervous influence."

B. What have you to say about the action of the will in vital motion?

A. I certainly do not look upon the will or any other mental faculty as a mere sign of functional activity in any part of the nervous system. I look upon the brain as the product of the mind, rather than upon the mind as the product of the brain. I find that voluntary muscular contraction is attended by a disappearance of electricity in nerve and muscle, and I suppose that the will has brought about the contraction by discharging, so to speak, a certain amount of electricity in each voluntary act. I do not change the point of view at all.

B. How do you account for the rhythmical action of certain nerve-centres?

A. By supposing that the electrical charge of the rhythmical nerve-centres "runs down" more quickly than that of the non-rhythmical nerve-centres. In the case of the rhythmical nerve-centres within the heart, I suppose that this charge "runs down" more quickly than in the case of the rhythmical nerve-centre or centres which have to do with respiration, and that, for this reason, the movements of the heart are more rapid than those of the chest. Comparing the rhythmical nerve-centres with the non-rhythmical, it would seem that the inhibiting action which prevents contraction, is more prolonged in the case of the latter centres, than in the case of the former. There is, however, no impassable line of separation between the two, for, as is seen in the case of paralysis agitans, or chorea, or disseminated spinal sclerosis, the action of the brain or spinal cord, which is non-rhythmical under ordinary circumstances, may become rhythmical under extra-ordinary circumstances. In these cases, it would seem that the electrical charge of the brain or spinal cord, diminished by degeneration or by an insufficient supply of red blood, "runs down" more quickly than it ought to do, and that, for this reason, nerve-centres which are non-rhythmical naturally become rhythmical. That is all. And if the rhythmical nerve-centres within the heart, or the respiratory nerve-centre or centres are more highly developed, or more vivified by red blood, it may be supposed that the electrical charge of these centres will "run down" less quickly, and that the effect of this change would be to retard the beatings of the heart and the heavings of the chest. In a word, it is not difficult to bring the non-rhythmical and the rhythmical nerve-centres into the same category, and at the same time to see that all nerve-centres have, so long as their electrical charge is kept up as it ought to be, the inhibitory action which is supposed to belong only to certain special inhibitory centres.

B. How do you account for persistent spasm upon this electrical hypothesis?

A. Without any great difficulty. Indeed, all that is necessary is to suppose that the electrical charge of the nerve-centres which have to do with the muscles in the case of persistent spasm, is scarcely kept up at all— that it "runs down" almost as soon as it is made manifest, and that, for this reason, the muscles have never time to pass completely out of the state of contraction into which they are thrown by the induced currents which are developed by any sinking or rising movement in the electricity of the nerve-centres connected with the muscles.

B. How do you account for the fact that nerve or muscle is thrown into a state of action by what is called "mechanical irritation?"

A. By supposing that the so-called "mechanical irritation" has led to the development of induced currents in and around muscle or nerve by disturbing the electrical equilibrium of the muscle or nerve. Any such disturbance, however brought about, must issue in the development of such induced currents; and a very little disturbance will serve for the purpose, for, as Faraday points out, induced currents are developed by simply moving a body which is not electrified or magnetized to or from a body which is electrified. In a word, I have no occasion to suppose that a vital property of irritability in nerve or muscle has been irritated or stimulated by the so-called mechanical irritant or stimulant, for all that has to be done in order to explain the phenomena which are referred to "mechanical irritation" is to call in the help of the induced currents which are developed under the circumstances.

B. If the coatings of the fibres of nerves and muscles are virtually leyden jars which are charged as such jars are charged during the state of rest, you can have no difficulty in understanding how it is that nerves and muscles are capable of retaining their electricity, and why the living system is kept in that state of tension to which the name of "tone" is given. I can see that the nerves and muscles can easily retain their electricity in this way: I cannot well see how they could do so in any other way. I can also see that the "tone" of the system may be maintained, partly by the active elongation of the muscular fibres which is kept up by the mutual attraction of the opposite charges on the two surfaces of the coatings, and partly by the mutual repulsion which must operate between different fibres and cells, because the exterior of these fibres and cells are similarly electrified with a + charge.

A. And, in addition to all this, you may also see if you take the trouble to look, that you need not continue to bother yourself by speculating about the origin of muscular force. The notion that muscular force is the result of a functional activity in muscular tissue, the amount of which is to be measured by the waste of tissue which finds expression in the quantity of urea and other nitrogenous compounds in the urine, is exploded. You now know that the amount of muscular action is to be measured, not by the elimination of urea, but by the exhalation of carbonic acid—that the amount of urea is practically the same on a day of work and on a day of rest: and, knowing this, you can scarcely continue to believe that muscular tissue is resolved into muscular force as it wastes under the disintegrating action of oxygen. Nor need you continue to talk about muscular force as the result of the transformation of heat. You know that the exhalation of carbonic acid is directly proportionate to the amount of muscular action. You know that the development of heat is directly proportionate to the exhalation of carbonic acid. But there is nothing in these two facts to justify the notion that heat is transformed into muscular force—nothing to justify the notion that electricity may not be developed, along with heat, in the combustion of force-fuel within the system, and that this electricity may not do the work which has been ascribed to muscular force in the way which has been pointed out. In a word, you may with little or no trouble satisfy yourself that "muscular force" and "nervous influence" must share the same fate, and that the only intelligible agent which is left in possession of the field is electricity.

B. It will, I expect, be a long time before I can school myself to avoid using words which imply a belief in the doctrine of vital irritability. I shall want a new vocabulary.

A. One day, in all probability, the words irritability, irritation, stimulation, and the like, will be replaced by other words which show that the idea of irritability is resolved into that of natural electricity; but the time is not yet come. And I am not going to bother you now by inflicting a new vocabulary upon you.

B. It must also be very difficult to get rid of the notion that convulsion or spasm is brought about by an over-active state of the circulation in one or other of the great nerve-centres. Not long ago I saw the temporal artery opened in a case of violent epileptiform convulsion on the supposition that the thing to be done was to take away blood from some part of the brain in which the circulation was over-active. In such a case, you, I suppose, would be disposed to increase the amount of blood in the arteries of the brain by keeping the head down, rather than to lessen it by raising the head, or by drawing blood. And so, too, in all cases in which convulsion or spasm, or tremor, or pain of the nature of neuralgia is a symptom, you, I suppose, would think it right to try and rouse the circulation to a state of greater activity rather than to carry out a contrary course of procedure. If, indeed, you are right in what you have said about vital motion a complete revolution in practice must be necessary in dealing with the cases in question.

A. I should certainly not bleed in cases of violent epileptiform convulsion. And probably I should make a point of depressing the head, for it is certain that a paroxysm of frequently recurring epileptic or epileptiform convulsion may often be shortened or stopped by taking away the pillow, or by letting the head hang over the edge of the bed. Moreover, I do not think it would be difficult to show, that, in the treatment of all cases in which tremor, or spasm, or convulsion, or neuralgia, is a prominent symptom, it is better for the treatment to be ruled by the view about which I have been speaking than by the view of vital motion that has so long been accepted as the true view. But I am not now going to deal with the therapeutical bearings of my argument, or to say more upon any *quæstio vexata.* I am tired with talking, and hungry to boot, so let us go and have lunch.

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